on recognisable geological factors, namely, the wearing out of the chasm of the Zambesi and the retrocession of its falls, we have an advance in our positive knowledge as to the remote age of the Palæolithic stone implement beds of South Africa. H. W. FEILDEN.

### Terminology in Electrophysiology.

TAKING the diagram given by Dr. Fraser Harris (p. 5), all writers will agree with him that A is positive plate and negative pole, and I believe that the use of the word "zincative" has gone some way to promoting this agreement.

Dr. Harris goes on to say that no loophole for confusion could be left if the qualifying words *externally* or *internally* were added, by stating, *e.g.*, that A is internally electropositive to B and externally electronegative to B.

I find three objections to this suggested clarification:—

(1) The expressions thus qualified are cumbrous, and must infallibly become abbreviated in current language by omission of the qualifying words.

(2) The expression "externally electronegative" contradicts the conventional use of the word "electronegative" which is attached to the plate and not to the pole.

(3) There is no provision for the complementary qualification to denote that a tissue is capable of being aroused to electromotive action, *i.e.* capable of being rendered zincative, *i.e.* zincable.

I freely admit that this convenient jargon offends the ear while arousing the understanding. I will gladly bury the words "zincative" and "zincable" when they have fully served their purpose as danger-signals that confusion is possible; but until we have agreed that active tissue shall be called "externally or galvanometrically negative" or "internally electropositive" I do not think loopholes for confusion have been closed. I should be satisfied with the old external word "negative" if it did not involve the conception of an internal "propagation of a wave of negativity," and the occasional misstatement that a wave of electronegativity is propagated through nerve and muscle. I should be glad to say that A is electropositive to B if I were not convinced that the prefix "electro" would be occasionally dropped to the further confusion of the reader accustomed to be told that A was (externally) negative.

So that, *en attendant mieux,* in order that there may be no confusion as to my own meaning that A is externally negative and internally positive, I say that A is like zinc or zincative. The parable, if there be parable, is intended to point out and avoid a confusion, but there appears to be an unfortunate tendency to confuse an indication of confusion with an introduction of confusion.

The physicist does not help us much. When he has appreciated the ambiguity of our physiological language, which is of physical origin, he supposes it to be no more than another case of the ambiguity familiar to us in the naming of accumulator poles, where, in order to avoid the perplexities that would arise from calling the same plate positive during charge and negative during discharge, the convention has become accepted always to call positive the plate that is connected with the positive pole of the charging battery or dynamo.

Our trouble is that there is among physiologists no accepted clear convention analogous with this convenient custom of miscalling the positive plate of an accumulator in a perfectly intelligible manner. Therefore again, when it seems particularly desirable to indicate seat of activity and direction of current internally as well as externally I still say "*zincative*" in order that there may be no mistake of meaning. A. D. WALLER.

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BOTH Dr. Harris (p. 5) and Prof. MacDonald (p. 28) somewhat misrepresent the use by physicists of the signs + and −. As applied to a closed circuit they are purely relative, each point being simultaneously positive to all points on one side of it and negative to all those on the other. The confusion arises from the fact that terms belonging properly to electrostatics were adopted long ago in describing the phenomena of the galvanic battery.

It is impossible to define direction in a circle—or any closed circuit—with less than four points—one lying in a different plane from the other three. In a circle drawn on paper, one of these is given, namely, the position of the observer behind or in front of the paper. If we put two others on the circle there is still ambiguity, for we may go from + to − in either direction, as in the armature coils of a dynamo; but with the use of three symbols the ambiguity vanishes. Thus *abc, bca, cab* indicate one direction, and *cba, acb, bac* indicate the other. As with formulæ containing an asymmetric carbon, the enantiomorph is given by turning the diagram over.

In diagrams of electric circuits some portion of the apparatus, either the source of E.M.F. or the place where it is being used, is tacitly taken as the third symbol, and the signs + and − put on either side of it to indicate which way the current flows. Dr. Waller's word "zincative," to anyone who knows how zinc behaves, indicates without possibility of error the direction of the current across the region that generates it, and the electrically-minded student may trace its course thence by arrows, remembering that while the circuit is closed the tail of each arrow is positive to its head, but directly the circuit is broken the whole of the side that ends with a head is positive to the whole of the side that ends with a tail.

To the physicist the terminology formerly used by physiologists was most confusing—in my own case it conveyed an entirely wrong impression until I had made an experiment with my own hands. It is most desirable that the anomaly should be removed, and in my opinion it may best be done by dropping the unsuitable terms positive and negative, and saying either that current flows from the more active to the less active part of a tissue or that the one is zincative to the other. GEORGE J. BURCH.

University College, Reading, November 11.

### Action of Radium Salts on Gelatin.

ON continuing the experiments detailed in NATURE of October 26, I found that lead and strontium salts produced the same results upon gelatin as was the case with radium, but the strontium "growths" were much less vigorous than the others.

On considering the results, it is seen that the metals named are those which form insoluble sulphates, and it occurred to the writer that the "growths" were simply a precipitate of some insoluble body formed by the action of the salts used upon the gelatin.

Various solutions of bouillon and gelatin were prepared, and to each were added a few drops of solution of radium or barium or lead salts, with the result that in each case a precipitate was obtained which on careful examination was found to consist of a sulphate, or at all events an insoluble compound, containing sulphur.

The precipitate produced by the radium salt was tested to see whether it was in any way different from that produced by the barium salt, but, with the exception that it was radio-active, it appeared to be similar in all respects. It was insoluble in strong acids, and gave a sulphide on fusion with sodium carbonate on charcoal, and qualitatively contained no other metal than barium.

In making the experiments, a few drops of the gelatin were placed on a glass slide, and particles of radium and barium salts added as described in the last communication. The "growths" appeared. Some solution of barium nitrate or radium salt was now added to the liquefied jelly. The usual precipitate appeared, and this was filtered off through a porous tube. The clear jelly was now tested with radium and other salts, and no growth could be seen even after seven days.

I think this proves very conclusively what the alleged "growths" are, viz. that they are nothing more than finely divided precipitates of insoluble barium salts. I have examined these precipitates with the highest microscopic power at my disposal, and cannot, in any case, perceive that there is anything of the nature of cell division occurring.

Of course, many pairs of particles may be found, but the grouping must be purely fortuitous.

As there is only a limited amount of matter in the gelatin which can be precipitated by the radium, a concentration occurs at the point of contact of the salt with

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the gelatin, and then a slow diffusion of the remaining salt takes place downwards, and this might give rise to the idea that the thing was really growing.

W. A. DOUGLAS RUDGE.

Woodbridge School, Suffolk.

### The Spectrum of the Positive Rays (Canal-Strahlen).

IN former publications (*Ann. d. Phys.*, vol. xiv., p. 524, 1904; vol. xvi., p. 490, 1905; "Die Elektrizität in Gasen," Leipzig, 1902, pp. 446, 457) I have expressed the opinion that the carrier of the line spectrum of a chemical element is the positive atom-ion, while, on the other hand, the band spectrum is due to the re-combination of positive atom-ions with negative electrons. From this it would follow that the particles of the positive rays (being positive atom-ions) should emit the line spectrum of the gas in which they are produced. Moreover, since these particles possess a considerable velocity, their spectrum lines, observed in the direction of the rays, should have a position and breadth differing from the position and breadth when the lines are observed in a direction normal to the rays. Again, since the positive rays ionise the gas traversed by them, re-combination of positive and negative ions must take place in their path; the gas traversed by the positive rays must therefore emit the band spectrum, which will be superposed on the line spectrum due to the positive rays themselves. The lines of the band spectrum must have the same position whether they are observed in the direction of the positive rays or in a direction at right angles to them, inasmuch as their carriers do not possess the velocity of the positive rays.

I now state briefly the results of an experimental spectrographic examination of the light emitted by a gas traversed by positive rays. (1) Nitrogen shows simultaneously the band and the line spectrum, hydrogen shows simultaneously the series spectrum ($H\alpha$, $H\beta$, . .) and the many lines spectrum. (2) The line spectrum emitted normally to the positive rays and the line spectrum emitted in the direction of the rays are different; the former shows in hydrogen sharp lines of the known wave-length, while the latter shows these "stationary" lines, and besides them, on their ultra-violet side, new widened lines ("displaced" lines). (3) This displacement is greater when the velocity of the positive rays is greater. (4) The lines of the band spectrum (many lines spectrum) have the same position and breadth whether they are observed in the direction of the positive rays or in a direction at right angles to them. A full account of the investigation will be published shortly.

Göttingen, November 3. J. STARK.

### Replicas of Diffraction Gratings.

KINDLY allow me to correct a statement contained in your notice of Mr. R. J. Wallace's replicas of diffraction gratings in your issue of November 2 (vol. lxxiii. p. 21).

It is there stated, as also in the *Astrophysical Journal* from which the extract was taken, that I first flood the grating with oil in my method of producing replicas. This I may say I have never done except when making experiments, my procedure being exactly the same as Mr. Wallace's, viz. to flood the grating direct with the clarified celluloid solution, dry it in much the same way, but using special precautions to ensure perfectly even drying, stripping and mounting in a similar manner to Mr. Wallace, but leaving out the gelatin coating, which in my opinion is quite unnecessary.

I beg to enclose you one of Mr. Wallace's first quality (average) replicas, kindly sent to me by him in exchange for one of my own, as also a couple of mine for comparison. The great difference to be noted in their surfaces and performance is due to the peculiarities in the surfaces of the original gratings, one of my own replicas having a brightness in the first spectrum on one side of at least four times that of the other, and twice that of Mr. Wallace's replica.

The grating from which the very bright replica is taken is a "Rowland" of 14,438 lines to the inch, and was formerly the property of the late Dr. Common. The original of the other is a very beautiful specimen of recent work on the Rowland engine, 15,038 lines to the inch. Now, whilst the latter when mounted on parallel plane glass gives comparatively feeble spectra, when mounted on prisms for direct-vision purposes, and tilting the prism to the angle required for the minimum deviation for the diffraction spectrum, first order, the brightness approaches that from the "Common" grating, whilst its much greater freedom from scattered light renders it very suitable for prominence and similar work, the dispersion being about equal to five 60° flint glass prisms in the centre of the spectrum, and decidedly greater at the red end.

This increase of brightness is, of course, attributable to the form of the grooves, less interference being produced under the latter condition, and this notwithstanding the increase in dispersion.

It may be of interest to some to know that I have succeeded in mounting these grating films on a perfectly flattened ring of glass, so that, by avoiding the use of glass as a base, light of very short wave-length can be examined by this means, either in the one case by transmission to about $\lambda$ 2600 or by reflection to as low as $\lambda$ 1850, and possibly lower. (The discovery of this reflective property for ultra-violet light was made by Mr. Morris-Airey, of the Victoria University, last year.) In order to examine by reflection either a partial vacuum is created behind the film when mounted on a glass ring or the film is mounted on a concave surface, which, although not giving the lines of the grating their true form, gives very fair resolution.

I have also succeeded in making concave replicas practically as perfect as plane ones, by rotating the grating during the drying process at such a rate that the paraboloidal curvature of the solution was practically the same as that of the grating. Anyway, the difference is so slight that when dry no rings can be seen on examining it by monochromatic light before the film is stripped from the grating. The difficulty of silvering these replicas satisfactorily has, however, prevented further progress, for the present at least.

In justice to Mr. Wallace I ought to say that, in a reply to a letter from me, he states he obtained his information from a patent I once took out in connection with the application of these grating replicas to colour photography; but the method there described is not the one I have adopted in making my replicas.

THOMAS THORP.

Whitefield, near Manchester, November 6.

THE article referred to by Mr. Thorp was, as mentioned in its first two lines, simply a *résumé* of Mr. Wallace's article in the *Astrophysical Journal*; and the statement corrected by Mr. Thorp was taken from that journal. It may be said, however, that the three gratings sent by Mr. Thorp have been compared, under similar conditions, and the results are in full accordance with the descriptions given above.

The second "Thorp" replica (15,120 lines) is a beautiful specimen in appearance, having none of the mottling which appears on the 14,438-line copy, but its first-order spectra are excelled in brightness by the first-order spectrum on one side of the latter, which is a similar replica to those that have produced such remarkably good results in eclipse and other observations when attached to just an ordinary hand-camera or a simple modification thereof.

THE WRITER OF THE NOTICE.

### Aurora of November 15.

AN extraordinary aurora was seen here on November 15 at 6 p.m., in appearance something like a "stormy" sunset. The lower part of the aurora was illumined with a bluish-green light, and had an altitude at the centre of about 10°. Above this, extending for a further ten or fifteen degrees, the sky was brilliantly illuminated with streamers of a rosy red colour. I did not wait for its disappearance, but at 9.30 p.m. red streamers were visible in the N.N.W.

I looked for the Leonids this morning continuously from 1.20 to 2.35 from the vantage ground of the Dartmoor hills (1500 feet). I saw only one Leonid at 2.30, but the moon would prevent small ones being seen. The night was perfectly clear.

ROWLAND A. EARP.

Buckfastleigh, S. Devon, November 16.

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